



University of Groningen

Letters to the Editor

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Published in:
Isis

DOI:
[10.1086/498603](https://doi.org/10.1086/498603)

IMPORTANT NOTE: You are advised to consult the publisher's version (publisher's PDF) if you wish to cite from it. Please check the document version below.

Document Version
Publisher's PDF, also known as Version of record

Publication date:
2005

[Link to publication in University of Groningen/UMCG research database](#)

Citation for published version (APA):
Kubbinga, H. (2005). Letters to the Editor. Isis, 96(4), 622-623. <https://doi.org/10.1086/498603>

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LETTERS TO THE EDITOR

TO THE EDITOR:

I appreciate the opportunity to respond to Mary Mosher Flesher's review of my *"Fighting for the Good Cause": Reflections on Francis Galton's Legacy to American Hereditarian Psychology* (Isis, 2004, 95:316–317).

Most of the problems I have with Flesher's review stem from her misinterpretation of two terms I used in the book, "hard heredity" and "soft heredity." I felt I had supplied the long-established definitions in sufficient measure—for example, hard heredity denotes the impermeability of hereditary transmission to both environmental and experiential factors (acquired characters) and to birthmarking as well (p. 50). I also juxtaposed the sociopolitical corollaries that have issued from hard and soft heredity subscriptions in a fashion meant to supplement the definitions given (pp. 53–54). Nonetheless, Flesher took hard heredity to be "the idea that inheritance is the primary determinant of intelligence" and soft heredity as "a more scientifically accurate theory affirming the mutual influence of inheritance and nurture." Flesher seems hereby to mistake terms reserved to the phylogenetic debate over the susceptibility of hereditary transmission to external interference (Weismannism vs. Lamarckism-plus) as terms to be employed in the ontogenetic debate as to whether heredity or environment most influences development during the course of the individual's life (the more familiar nature–nurture controversy). Such misapplication looks to have dictated an unfortunate misreading of my message and purposes.

For example, I advance my interpretation of the famous pangenesis affair at some length (pp. 56–66), arguing that the American psychologists I am considering as Galton's observers would have had good cause to see him as secretly intent from the first on overthrowing Darwin's "provisional hypothesis" as a polar antithesis to hard heredity and, thus, a hazard to his newborn eugenics enterprise. Nowhere did I intend to imply what Flesher somehow infers—that Galton *failed* to adopt his cousin's hypothesis as a "means to move to a more scientifically accurate theory."

Flesher misconstrues one of my discussions of the American educational psychologist Edward L. Thorndike in similar fashion. In attempting to demonstrate that American hereditarian psychologists were more conscious of the deficien-

cies of Galton's and Karl Pearson's evidence than they were typically willing to admit, I cite two of Thorndike's considerations of Pearson's correlational study of the physical and mental resemblances of young siblings, an investigation published in 1901–1903 and widely regarded as a major bulwark of the hereditarian argument. In 1903 Thorndike endorsed Pearson's study unreservedly, as "perhaps the most valuable research in educational psychology yet made" and logically irresistible in its conclusions. In 1914 Thorndike painstakingly dismantled Pearson's study, stressing at the end its logical absurdity (p. 100). I explain this remarkable volte-face as merely Thorndike responding in kind to a recent attack made on the American school of eugenics by Pearson's school, a classic case of expediency trumping consistency, and a good indication of the critical skills hereditarian psychologists could apply to their own body of proofs when so inclined. Flesher elects to read this reversal as showing instead that Thorndike "does not fit snugly into the mold of an unquestioning supporter of the eugenics of Galton and Pearson," "given that [he] modified his position on heredity in 1914 from 'hard' to 'soft.'" Such a reading of Thorndike's turnabout would be untenable even with the misapplied terms converted into what Flesher seems to mean by them. Thorndike could hardly have gravitated from a hereditarian to an environmentalist position (in the nature–nurture debate) by 1914 and have earned his enduring reputation as the longest-term and most consistent hereditarian of all his American psychologist peers.

Finally, by objecting to the book's devoting "almost double the space to Pearson that [it] does to the three American hereditarians," Flesher seems also to mistake its scope. The reason I give for so extensively considering Galton's and Pearson's development and promotion of the eugenics program is that the study seeks to illuminate openly visible features in that decades-long process that could have encouraged these psychologists to perceive Galton as covertly opposing, through his formulations of eugenics doctrine and policy, the wide-open meritocracy and wholesale democratization that he and his nation saw hurtling toward them. As I explain in several places (pp. 1–2, 50, 93), the study is about what Galton (and Pearson) gave to these

American observers in terms of doctrine, evidence, and example; it is not about what these legatees did with such a bequeathal—which, as I also explain, is the subject of another work, in progress.

GERALD SWEENEY

IN REPLY:

A brief book review and this post-review exchange is a small space in which to discuss the complicated arguments in Gerald Sweeney's book *"Fighting for the Good Cause": Reflections on Francis Galton's Legacy to American Hereditarian Psychology*.

Sweeney is correct that I did not in my reading of his book, with its many twists and turns, stay within the narrow confines of the phylogenetic terms "hard" and "soft" heredity. In other words, I did not avoid the term "nature–nurture," which rightly belongs with ontogenetic development. However, in his short, prominent introductory chapter Sweeney himself cites Thorndike's praise of "Galton's rigidly hereditarian ruling on the nature–nurture issue" (pp. 1–2), and he opens the book's last chapter on the American hereditarian psychologists by announcing the importance of heredity over environment for ontogenetic development (p. 93).

Sweeney does argue that Thorndike's 1911 scientific critique of the inadequacy of Pearson's data supporting hard inheritance was provoked into print only by a previous attack on him by a Pearson adjutant, something he already knew but would have kept surreptitiously quiet (p. 100). I apologize.

The author interprets Galton's interest in "hard" heredity as only political—indeed, surreptitiously political, and thus requiring a scientific cloak. In other places in the text, Sweeney casually notes that Galton did make scientific contributions (p. 1). In the case of the pangenesis dispute, however, Sweeney argues that Galton could not accept pangenesis because it would undermine his "hard" hereditarian position, the fundamental support for his eugenics, his only real interest being political support for the oligarchy. Therefore, Sweeney does not accept that Galton had any scientific concerns in spending a number of years doing experimental work with blood transfusions to disprove that aspect of Darwin's theory of pangenesis. This was only a sham to keep peace with Darwin (pp. 58–66). I apologize for misrepresenting Sweeney's position.

Sweeney also states that I do not understand the scope of his book. In the "Acknowledgments,"

the author concedes that much that is conjecture here will receive confirmation in a second book on which he is working (p. x). And true, the language of the present book is full of conjectures (probables, could haves, might haves), equivocations, and ironies. A book should stand on its own.

The idea of a proposed second book may help, however, to explain the presence of an "Abstract" at the beginning of this text (as though it were a journal article), the short introductory chapter, and the lack of a satisfactory concluding chapter. Sweeney claims to have shown that American hereditarian psychologists, publicly admiring Galton as an inspiration for themselves, did not do so on the basis of the quality of his science ("Abstract"). Therefore, the book is only about Galton's eugenic program as amended by Pearson and establishing that Galton's evidence gathering was "something other than scientific inquiry, even by the standards of his day" (p. 12).

The last chapter, however, makes three points about when and how the American hereditarian psychologists would have known about Galton's work. It seems that this information might have better been placed in a second book than as the concluding chapter of the first book. Placing more of the important material of the earlier chapters in the text rather than in the footnotes would have made a sufficiently long book. The second book is to show how the Americans, in their classic period of influence (1903–1930) and in their own context, were "instrumentally informed by their perceptions of Galton's ulterior purposes," purposes they clearly perceived (p. 1). I believe that Sweeney's theses are overstated, but then I have not seen the second book, which is to confirm them.

The experience of reading this book leads me to conclude that it was written directly for academic experts in the field of phylogeny and not, as its primary concern with political motivations and influence might indicate, for those who have a general interest in British cultural history in the second half of the nineteenth century and its background in science.

MARY MOSHER FLESHER

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TO THE EDITOR:

Any historian of science justly takes pride in seeing his book reviewed in *Isis*. Sometimes, however, even in *Isis*, a *maledictum* slips in in the place of a balanced judgment. Pierre Laszlo's

review of my book *L'histoire du concept de "molecule"* (Isis, 2004, 95:728) is a case in point. He does not tell the readership what this three-volume work is all about but just points out what, in his opinion as a chemist, ought to have been in it, without even verifying whether or not these "musts" are in the book. What becomes clear, to say the least, is that the history of science does not necessarily coincide with the history of chemistry.

My book describes the birth of the concept of "molecule" in the seventeenth century, against the background of the various theories of matter that have played a role from antiquity onward. The ancient atomists are there, and Plato has a chapter of his own, while Aristotle and the Greek and Latin Peripatetic tradition are dealt with in depth. Epicurus's and Lucretius's theories are of course analyzed with particular emphasis, because the first molecular theory was based on their neo-atomism. Around 1620 two natural philosophers coined the concept: the Dutchman Isaac Beeckman (1588–1637) and the Frenchman Sébastien Basson (ca. 1580–after 1621). From 1620 onward the molecular theory spread across the sciences. It was, of course, not a question of either chemistry or physics alone; crystallography and mineralogy were also involved, as were biology and medicine. In a way the development culminated in the physics of Laplace, a physics based on the recently proposed theory of three states of aggregation: all natural phenomena, even heat and electricity, were postulated to be of a molecular kind. Therefore I could rightly claim that *molecularism* had taken the place of *atomism*, at least in a first approximation of the problems involved. This was not an ill-based claim: I read several papers on this topic—for example, at the annual meeting of the History of Science Society held in Pittsburgh in 1999. Through the nineteenth century the molecular theory was charming still other sciences. Even philosophy came under its spell. The *philosophie positive* of Auguste Comte indeed appears, on an attentive reading of Comte's *Cours*, to be a derivative of Laplace's exclusively molecular physics. In two decades of intensive research on source materials, my book grew to cover 1,890 fascinating pages in three volumes. The reader will find there, I do hope, most, if not all, successive connotations of the molecular idea. Suffice it here to mention its first name: before the word "molecule" existed, the notion was called *homogeneous* (*physicum*) by its inventor, Isaac Beeckman (ca. 1620), a word graciously borrowed from Euclid's *Elements* (def. 5.3).

One of the results of my in-depth research is

the finding that the Dutchman Herman Boerhaave was but a poor chemist, who did not notice the brilliance of the (molecular!) chemistry of Stahl, later Lavoisier's great opponent. Ever since I have argued against Dutch colleagues—fellow countrymen of mine—when they, somewhat shamelessly, dare to claim the contrary. In much the same spirit I combat the systematic overrating, in France, of Ampère's chemistry: perhaps Ampère was indeed to become a skilled experimental physicist in the 1820s; nonetheless, his paper of 1814 cannot bear comparison with Avogadro's paper of 1811, that genuinely brilliant piece of wonderful, straightforward chemistry (in my book Avogadro deservedly got some fifteen pages, Ampère just a footnote). Talking about the "hypothèse Avogadro-Ampère," as is customary in French textbooks, is perhaps permissible for pedagogic reasons; omitting the name of Avogadro, in 2004, while hailing Ampère's "landmark 1814 article," as Laszlo does in his review, is both a chemical and a historiographic error, if not a scandal. His nonspecific (!) credit for the origin of the notion of a gram molecule to Alexander Crum Brown (I think he meant Crum Brown's 1883 article "Molecule" in the ninth edition of the *Encyclopaedia Britannica*) is very much of the same kind.

Laszlo suggests, without giving a page number, that I would claim that Perrin was the inventor of the gram molecule concept. If he had indeed consulted my book, he would have found (Vol. 3, pp. 1084 n 1, 1180) that his fellow countryman, my friend Yves Noël, demonstrated in the early 1980s that the gram molecule concept, and more particularly its abbreviation "mole," was introduced by Wilhelm Ostwald (1893). It was based, not on Crum Brown's singularly irrelevant article in the *Encyclopaedia Britannica*, but on several decades of fine chemistry and, more particularly, Thomsen's thermochemistry—all described in my book (Vol. 3, p. 1089f.).

I passed the Ph.D. and the *habilitation* in Paris under the aegis of the Centre Alexandre Koyré (EHESS) and still consider myself—if only by "action at a distance"—as part of the French inner circle.

HENK KUBBINGA

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IN REPLY:

Henk Kubbinga's letter shows how opinionated a scholar he is, which was already a major problem with his book. To be opinionated may be

fine; to be biased is not a virtue. Kubbinga's letter also displays the one-upmanship ("I could rightly claim," "my in-depth research," "dare to claim the contrary," "part of the French inner circle") that greatly mars his book. The narrow-mindedness is illustrated here by the two debatable statements relative to Ampère and to the Laplace–Comte connection.

Ampère's chief merit was his distinction between—to use modern terminology—the atom and the molecule (see M. Scheidecker-Chevalier and R. Locqueneux, "La théorie de la combinaison chimique d'A.-M. Ampère," *Revue d'Histoire des Sciences*, 1994, 47:309–352; and Scheidecker-Chevalier, "L'hypothèse d'Avogadro [1811] et d'Ampère [1814]: La distinction atome/molécule et la théorie de la combinaison chimique," *ibid.*, 1997, 50:159–194). Avogadro saw his *molécules constituantes* as the fundamental chemical units, not the *molécules élémentaires*,

with only a mathematical essence (Marco Ciardi, "Amedeo Avogadro's Concept of the Atom: Some New Remarks," *Ambix*, 2001, 48:17–24). Dumas—to mention a single chemist whose role was significant in the development of atomic theory—was influenced by Ampère's, not by Avogadro's, paper. Avogadro was arguably in the tradition of Lavoisier, outside the fold of Berthollet's physics.

Auguste Comte had little interest in chemistry, which he considered a systematic kind of knowledge—descriptive, minimally predictive, a mere collection of observations—whose only contribution to positive science was its nomenclature. To claim that the *Cours de philosophie positive* derives from molecular physics is a misreading.

I stand entirely by my review.

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